THE JOURNAL OF PHILOSOPHY

PSYCHOLOGY

AND

SCIENTIFIC METHODS

VOL. III. No. 25: DECEMBER 6, 1906

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PUBLISHED FORTNIGHTLY BY

THE SCIENCE PRESS

LANCASTER, PA.

AND

NEW YORK: SUB-STATION 84

VOL. III. No. 25. DECEMBER 6, 1906.

THE JOURNAL OF PHILOSOPHY
PSYCHOLOGY AND SCIENTIFIC METHODS

A DEFINITION OF EXPERIMENTATION

THE question of the meaning of experimentation is brought repeatedly before us, if we read any criticisms of method by the scientific investigators themselves, or, indeed, if we stop to consider the reason for the terms and subdivisions of the different sciences and to ask why experiment plays so large a part in some and is so little used in others. Such phrases as 'the treacherous path of speculation instead of the safe way of observation and experiment'[1] or as 'those experimental investigations upon which the growth of science depends,'[2] and the sad note in the statements made by a distinguished geologist that 'there has been during the last few years a large accumulation of geological evidence, a little new speculation, but practically no new experimental work,'[3] and that geology is 'only beginning to enter the experimental stage';[4]—these, and the many other similar references to be met with, all suggest that for some reason the ideal of science is to build on experiment primarily if not entirely.

When we ask what experimentation is or what an experiment is, and much more when we ask how it is that experiment achieves its result, the answers are not so easy to find as we might hope, particularly if we turn for them to the scientific investigators themselves. Experimentation and observation together make up our means of appeal to the external world. That is clear. They connect us in some way with the phenomena there, make us master in part of that world of fact that yet we never feel as within our control. What, however, is the distinction between the two, how are we to tell an experiment from an observation? Dr. L. F. Barker, of Johns Hopkins University, stated in his address before the Massachusetts Medical Society in June, 1905, that their difference was one of relative exactness. In the report of another physician, we have again

[1] Carl Schorlemmer, 'Rise and Development of Organic Chemistry,' p. 4.

[2] Thomas H. Huxley, 'Collected Essays,' Vol. I., p. 55.

[3] *Nature*, August 24, 1905, p. 408.

[4] *Ibid.*, p. 413.

the phrase 'exact experimental evidence.'[5] But Lord Kelvin seems to refer to something different from exactness in saying 'their eyes to observe accurately and their hands to experiment, in order to learn more than can be learned by mere observation.'[6] Experiment to him promises new rather than merely more exact knowledge. Pasteur's statement that experimentation 'demands besides certain natural qualities a long practise which naturalists have not generally acquired nowadays'[7] is rather non-committal, but certainly does not give exactness as the unique achievement of the experiment. Huxley is satisfied to call it 'observation under artificial conditions,'[8] which is perhaps something like what Titchener means in defining experiment as 'simply an observation made under standard conditions,'[9] but it is certainly not a mere repetition of the demands either for measure and exactness or for the use of one's hands. We have, finally, Mill's well-known statement that "we may either find an instance in nature suited to our purposes, or, by an artificial arrangement of circumstances, make one. There is, in short, no difference in kind, no real distinction between the two processes of investigation (observation and experiment). There are, however, practical distinctions."[10]

The demand for a clear understanding of the word experiment in the physical sciences is made stronger when we remember that its use is not confined to those. The mathematician and logician insist that they, too, experiment when, for instance, they try number after number as the root of a given equation or develop the result of one after another of all imaginable combinations of postulates in the search for a new one that is useful and significant.

The suggestions at definition are purposely taken from a rather wide range of time and fields of investigation. The fact that they are not in obvious agreement does not surely mean that their authors held ideas of the proper use of the word experiment which were fundamentally different. It is possible that an investigator in a given field might take a characteristic of the result that could be gained only through experiment there as an invariable and necessary characteristic of experimentation in all fields. In the hope of bringing to light a common idea at the basis of all these special meanings for experiment, I have attempted to classify the methods of scientific investigation in such wise that experimentation should be exemplified

[5] J. J. Kilyoun, 'Bacterial Content of Railroad Coaches,' July, 1905.

[6] Lord Kelvin, 'Popular Lectures and Addresses,' Vol. II., p. 478.

[7] René Vallery-Radot, 'Life and Works of Pasteur,' translated by Lady Claud Hamilton, Vol. I., pp. 125–6.

[8] *Loc. cit.*, p. 66.

[9] E. B. Titchener, 'Primer of Psychology,' p. 26.

[10] J. S. Mill, 'A System of Logic,' Vol. III., Chap. VII., § 2.

in all the examples falling under one heading of my classification and in that way be contrasted with the methods belonging to the other headings. I use the term experimentation rather than experiment designedly because a new question enters if we ask how the single experiment is distinguished from the group of experiments or phase of an experiment.

The classification I finally adopted is the following. The comments which follow the classification may perhaps make its underlying principles clearer. It will be seen at once, however, that the different classes are not mutually exclusive, that is, the examples are chosen only because each calls for the unique element of method which that heading names. Application of any one of these methods would usually involve the use of some other as well.

A classification of methods used in the physical sciences in gaining data from experience with the natural world:

A. *Observation of the outside.*—The investigator does not consciously and voluntarily interfere with the phenomena he studies. I. Simple observation. The investigator merely observes as does the 'every-day' man. II. Complex observation. The investigator supplements the range of his sense organs. Examples: use of microscope, telescope, stethescope, X-ray machine; dragging the bottoms of lakes and oceans; bringing a compass near a magnet; spreading sand over a membrane to observe a faint sound vibration. III. Measure. The investigator supplements his powers of accurate comparison. Examples: measurement of length, time, strength of sound by modified stethescope; counts of number of bodies of different sorts in a drop of blood; counts of number of stars in a given region of the sky. IV. Transference (in position and by copymaking). The investigator supplements the accuracy of his memory or serves his greater convenience. Examples: moving specimens into the light; making photographs; making accurate drawings; staking out the outline of bands that precede the eclipse of the sun; use of the needle and smoke drum; use of preservatives for specimens.

B. *Observation of the inside.*—The investigator consciously and voluntarily isolates certain aspects of phenomena from their natural surroundings on the assumption that he keeps those aspects essentially complete and undisturbed. I. Material classification. The investigator isolates certain portions of a total experience, with the hands. Example: dissections. II. Mental dissections. The investigator isolates certain aspects of a total experience. *a.* Record-taking. By confining his attention to a portion of the qualities or conditions present. Examples: medical records; geologists' notes as to the fauna and flora present in a given country. *b.* Diagram-making. By confining his attention to some of the relations pre-

sented. Example: diagrammatic drawings of structure made by the anatomist, the zoologist, the crystologist.

C. *Experimentation.*—The investigator consciously and voluntarily alters the conditions of the phenomena studied.

According to this classification the distinction to be made between observation, on the one hand, and experimentation, on the other, is, as Mill says, not one 'in kind,' but a 'practical' distinction only.

A good typical instance of experimentation of least complexity is found in the case of Pasteur's experiment upon the condition of air in different places.[11] He arranged a number of sealed flasks containing some substance in which bacteria thrive. These he opened in turn to the air at different points, some in the low country and some high among the snow peaks of the Alps. He afterwards examined the contents to discover at which spots, if any, the air which had entered the flasks had not brought with it bacteria. Such a set of processes is an experiment, because the conditions which the substance within the bottles met were made distinctly artificial. Pasteur did not find substances isolated, exposed to the atmosphere at a single given spot and then isolated again. True, he himself did not touch the contents of the bottles directly in altering their conditions, but yet he did so indirectly by removing and replacing their seals. Experimentation in many cases calls, of course, for much more elaborate apparatus.

Such processes are to be contrasted with the two kinds of observation. First, with those methods which, however elaborate, are used with the intention merely of getting a better view of the object. So far as that particular object is concerned we might imagine here that the investigator's hands are tied behind his back. Besides the careful watching needed as an element in all investigation this class includes the use of various magnifying and measuring instruments and similar contrivances, and as well different forms of copying, I think. All photographs do not stand on the same basis here. For the geologist they serve as illustrations for communication and thus are not a part of the methods of science, but rather of its means of promulgation. The photographs of the astronomer, however, are taken not that he may show some one else what he has seen, but that he may himself compare the condition of a part of the heavens on one night with its condition on another, and that he may make measures to determine the size of the stars and their relative positions at his leisure and in a convenient manner. He uses the photographs and gains more from them than he could have gained, at least with the same amount of effort, from direct observation.

The name of the group representing the second form of observa-

[11] *Loc. cit.*, pp. 127–8.

tion would be dissection but that, as a pathologist suggested to me, many dissections are performed with the avowed purpose of creating specific artificial conditions in the subject examined. With vivisections, for instance, this is generally true. 'Material classification' was suggested by Dr. Royce as following Kemp's assertion that the essence of classification is the marking a group of things off from others unlike them, rather than the binding of like objects together. There we have the motive of this second kind of observation at work. In order to study them more closely we separate the heart and lungs of a rabbit, for instance, from their natural surroundings.

By the records referred to something is meant other than the descriptions used continually in communicating results. When the physician records he notes only certain aspects (such as temperature, pulse, etc.) which he considers significant, and entirely ignores other aspects of the patient (such as complexion and height, perhaps), assuming that they have no bearing on the nature and cause of the disease. As a result he gives us only, as it were, a skeleton, or perhaps the breath or the heart of a man, with no suggestion as to how these scraps are to be filled out to reproduce the original he studied. In the descriptions made purely for the sake of communication details are often omitted, but there they are left unstated either because, as perhaps with colors, they are implied in the names of the trees and the rocks given or because the writer himself has forgotten them. In making records the investigator is conscious that what he gives can not be found by itself, just as a nerve can not be found except it be taken from an animal body. In his record he isolates aspects of the situation before him from the other conditions that invariably accompany them, and believes that none of the relations annulled by such isolation have any influence upon the aspects he does report.

The classification may be objected to as a whole on the ground that these classes which I have distinguished from each other are not really distinct, so that many true cases can be produced as evidence of its insufficiency. In what I call simple observation, for instance, the mere fact that our attention is centered upon one part of the field rather than another alters the aspect of that field somewhat. We can not be sure in any given instance that we do not interfere with the object we study. It is true, also, that no one who has read of the weeks of drill and the great precautions taken against jar and dust at the time of making observations of the eclipse of the sun in August, 1905,[12] or of the care with which an astronomer must adjust his telescope and ward against imperfections in its action[13] can feel

[12] *Nature*, September 7, 1905. Letter by W. J. L. Lockyer.

[13] Cf. 'Annals of the Astronomical Observatory of Harvard College,' Vol. XXVI., Pt. I.; Vol. XVIII., No. VII.

that the scientist finds it an easy matter to be sure that he perceives the phenomenon exactly as it is, by means of his reenforced sense organs. Measures, too, must be made with great care, often with the aid of microscopes, and must be revised as new and more accurate instruments are invented. It would seem to take fully as much effort and thought and preparation to guard against interfering with the natural course of phenomena as to plan just how one should interfere. We must, as it were, greatly interfere with our own natural course of action in order to guard against intruding ourselves upon the object we observe. The same thing is, of course, to be said, perhaps with more force, in the case of all dissections. In isolating the specimen we have the greatest difficulty in keeping it intact. For these reasons the subjective qualification is introduced into my definition, the qualification that the interference must be conscious and voluntary. Whenever the investigator believes that he has not interfered, he observes.

The line cases and the complex cases which can be pointed out are many; so, too, are the perplexing cases that can be classified only after a more careful examination. The large number of line cases suggests, what I must readily admit to be true, that their proper name is borderland cases,—that the division between the classes is made by a stretch of neutral territory rather than by a definite narrow fence.

We have, for instance, an investigation which Lord Kelvin describes that offers some difficulty.[14] In this the experimenter placed his head between the poles of a horseshoe magnet to determine whether he would become thus directly aware of the condition which made a piece of copper slip through the space 'as if through mud.' The pure observer goes to a selected spot to watch the action there. The operation in this instance could be so described. The investigator put his head in a certain spot in the hope that he might observe the conditions there. What this investigator wishes, however, is to learn the effect of certain conditions upon his nerves (or muscles, perhaps) other than those of the senses which are properly called into play in simple observation. He plays a double part, as it were. He puts himself in the particular spot more that he may, if possible, become affected by the conditions there than that he may watch the play of those conditions among themselves as the geologist watches the eruption of a volcano. Thus this would seem to me to be an example of experimentation.

For a different reason the case in which Malus held the crystal up to observe its effect upon reflected light is perplexing.[15] He

[14] *Loc. cit.*, pp. 127–8.

[15] 'Life and Works of Pasteur,' as given, Vol. I., p. 345.

altered the place of the crystal and so brought light to play upon it. When the zoologist pulls his subject into a better light as he dissects it, or reflects light upon a microscope slide, we should not say that he thereby experiments upon it. Malus placed his crystal in the light and looked through it, much, it would seem, as one looks through a microscope. That he might have chanced to lay it on a window in such a way that the same effect was produced, and might then have observed the effect, does not, it is true, bear on our point because in such chance action the change of condition would not have been both consciously and voluntarily brought about. But that he merely consciously and voluntarily changed its relation to light seems hardly enough to warrant calling the operation experimentation. The fact that the light was known to be reflected and not direct sunlight, however, and the fact that crystals and light react on each other in a manner not true in regard to animal bodies and light, introduce a sufficient element of really changed conditions to make it seem best to me to class this as on the borderland between experimentation and simple observation.

We have also to consider the references made by Wüllner in his section on acoustics[16] to general experience with musical instruments as to the effect of different sorts of bows, bridges and hammers upon the nature of sound. Musical instruments were by no means made for the purpose of testing the principles of acoustics. It is entirely immaterial to the experimenter whether he produces the sound or whether he chances to note it during the performance of a concert. Yet the instruments themselves are very carefully and very artificially constructed, and serve the purposes of science so exactly that if they had not been manufactured for the purposes of the musician the scientific investigator would have perforce constructed something very similar, for himself. These operations again seem to lie in a stretch of neutral territory between simple observation and experimentation.

These few examples have indicated that the different classes of the forms of observation do not represent stepping-stones from simple observation to experimentation, as those would suggest who insist that the essential of experimentation is complexity of apparatus. We can find examples that are in a neutral field between practically each kind of observation and experimentation itself. The only necessity for experimentation is conscious and voluntary interference with the object studied. This may be made directly with our own hands or may be accomplished by means of apparatus of various kinds.

The claim which such a definition makes for general acceptance

[16] A. Wüllner, 'Experimental-Physik,' Vol. I., pp. 874–5.

is that it offers a criterion for experimentation in all the fields of the natural sciences in which it is used, and rests on a characteristic of method which is significant and is not brought out by any other term we have. Further, the definition is in general agreement with most of the cases in which the word is commonly applied, although some investigations that have been called experiments would fall under one of my two classes of methods of observation.

It may be added that if such a definition of experimentation in the physical sciences is justified, the use of the term in psychology consistent with its use in these other sciences would not be limited to the field of accurate measurements. Those who insist that the psychological laboratory should be the place for such investigations only, might insist that all other investigations were necessarily imperfect and unfruitful, but they could not refuse to call them experiments.

FRANCES HALL ROUSMANIERE.

MOUNT HOLYOKE COLLEGE.

LINGUISTIC ABILITY AND INTELLECTUAL EFFICIENCY[1]

THE factors that go to make up an individual's general intellectual efficiency are perhaps too numerous and too complex for separate treatment by the experimental psychologist, if they do not even defy analysis by him. We generally assume that we can gauge with greater or less accuracy the mental qualities of an individual by a certain amount of purely empirical observation, but the exact sources of our judgment we often find difficulty in stating in a logical manner. An elementary distinction may be made as between intelligence and intellectuality; the former as an inherent quality and the latter rather as a matter of temporary information, for we often conceive of intelligence as the native aptitude for acquisition, and of intellectuality as representative of the amount acquired. Of course, the two are far from necessarily correlated. Whether or not the factor of intelligence, under this definition, be measurable, the writer hopes to discuss at some future time; the present experiments deal purely with the second quality, and then only with reference to actual and not at all to potential conditions.

Now if it be indeed impossible to subject to analytical experi-

[1] A report of an investigation begun in the spring of 1906. As it is not probable that the writer will have an opportunity to recur to this problem in the near future it has seemed best to publish the obtained results as they stand.

mentation the various factors that contribute to this efficiency, yet there is one single mental attribute, itself capable of many modes of experimental approach, that should show, if the proper method of approach be found, a quite close correspondence with general intellectual efficiency. This is the command over language, practically our sole experimental gateway to all the higher mental processes except attention. Nearly all the factors that constitute our intellectuality—as apart from our intelligence—come to us and go out from us through linguistic media. There seems to be no necessity for any detailed defense of this postulate; linguistic ability, if only the proper test of it can be found, should serve as a sound criterion of intellectuality. Such a measure, were it only precise enough, would be, needless to say, of considerable practical interest.

The memory tests are open to objection for the reason that they can be said to serve as a measure only of what per cent. of the individual experience has been probably retained. The fullness of this experience will vary, as well as its retention will vary, and this factor it is impossible to take into account by means of memory tests. They are dynamic rather than static measurements. In practise, however, it seems as though they failed even here, for the ordinary tests often fail to disclose in men of the highest intellectual ability any special logical or physiological retentiveness. Here also the varying factors of interest and attention are of prime importance, and equally impossible to control.

The association tests are also replete with technical defects. The difficulties in scoring would seem to make the ordinary test of uncontrolled association quite valueless, save for purely qualitative purposes. The special form of this experiment devised by Jastrow, in which the subject writes a fixed number of independent words as rapidly as possible, or preferably, writes as many independent words as possible in a given time, is much more objective in character, and probably altogether the best of the uncontrolled association tests. With proper technical precautions, this test ought, indeed, to afford a measure of the intellect, and be quite valuable where availability as a group test is a factor. As a test of intellectuality on a fine scale, however, the difficulties in scoring would seem almost insuperable.

The tests of partially controlled association, especially those of the synonyms and opposites type, are peculiarly open to this objection when the subject knows that he is being timed. For example, as an opposite of *simple* we obtain from one subject *difficult* and from another *complex*. The second subject takes half as long for the series as the first. We do not know what the first would have done had he taken as long as the second. It is the same difficulty as

with many of the motor accuracy tests; two unknown quantities and one equation. The test is valid only when every one has done his best regardless of time. As the education of the subject increases, so do the difficulties in scoring. We should probably disagree violently as to the proper manner of scoring individuals who respectively returned *perpetual, immutable, enduring, fixed, constant,* as synonyms of *permanent.*

Only where the association is so closely controlled as to render but one association possible can the time factor be justly taken into account. The chronoscope and voice-key lend themselves readily to the measurement of single associations. The most important work from this point of view is probably that of Cattell in the *Psychometrische Untersuchungen.* It is the most precise of all the methods, but it has certain technical limitations, and is rather cumbersome for practical purposes, to which the chain reaction is perhaps better adapted.

All valid tests of the higher mental processes should conform to one fundamental requirement; they should give the subject a certain definite thing to do, and admit of objective determinations of done, not done, or how well done. So far as association is concerned, only the time measurements of absolutely controlled associations really conform to this requirement. Otherwise arise the scoring difficulties that are fatal to validity. We can not well apply such an analogue of the method of average error to the higher mental processes because the scale is too ill defined. The association being under complete control, the results should contain only unquestionable rights or wrongs.

Several tests of this sort were devised and employed, among others, in Mr. F. G. Bonser's as yet unpublished investigation into the reasoning powers of children. Blanks were left in simple sentences for which there was only one appropriate word, the subject being required to supply it as quickly as possible. Two opposite words were printed, one above the other, in simple sentences, the subject crossing out that one which would make the statement an untruth. Logical and fallacious reasons were given for certain facts, the subject checking the good reasons. This last may be compared with the absurd sentence test, in which the subject checks those sentences which state as a fact a manifest impossibility; as, "The count paced up and down the garden reading the newspaper with his hands behind his back."

A simple test of the extent of vocabulary has been employed with good results by Kirkpatrick. The blank in the writer's possession contains the opposites test and a request for the definition of words of varying educational implication, but the most significant portion

of the test is a selected list of words most of which are considerably above the average difficulty, the subjects being asked merely to indicate the words that they know, do not know, or regarding which they are in doubt. It is unquestionably subject to certain errors in the individual, but for a large number of subjects these tend to compensate, and to remain constant as between groups. As between individuals, where the test of intellectuality has its greatest practical application, a strictly empirical demonstration of its validity will be desirable.

All of these tests, however, necessarily lack the objective character of a strictly motor test of linguistic ability, provided a satisfactory test of this nature can be devised. This it has been the writer's attempt to do. That only a very modest degree of success can yet be claimed the figures given will show, but the attenuated correlations which do appear indicate some possibilities in this direction.

Experimental psychologists have two ways of approaching problems of this nature. We can say, for example, ' let the *a-t* test measure perception of weight,' and then assume individual differences therein as indicated in the test. This procedure may be sometimes necessary, but it has been too frequently followed; certainly many of the conventional laboratory tests do not measure what they are given to measure, or else they measure a great deal besides. The other method is to empirically select individuals with known differences in the trait we wish to measure, and to seek a test which shows correlation with these differences. Before applying a theoretically devised measure to wholly unknown individuals, we should first test our tests.

The writer selected six individuals according to their degree of objective intellectual performance. These are denoted according to this degree, *A, B, C, D, E, F*. The differences between *A* and *B*, *B* and *C*, are clearly marked; that between *C* and *D* is largely subjective. *E*, however, is distinctly below *D*, and *F* below *E*; in fact, the difference between *E* and *F* may well be greater than that between *E* and *A*. The individuals are all adults, and the writer would estimate that they occupied, intellectually, a range of from the upper tenth of the distribution to about the middle.

As the problem was preeminently a search for a test many conditions are imposed that turned out valueless, and in some cases even impaired the significance of the figures that gave results. The writer does not wish therefore to be held accountable for resultless peculiarities in the presentation of the experiments, whose significance, or lack of it, could not have been determined in any other way.

The tests were concerned mainly with the speed of reading aloud. In the first experiment, passages I., II., III., IV., VIII., IX., X. are from scientific works, but only passage III. is at all complex in

TABLES

A, B, C, etc., *1, 2, 3*, etc. = subjects. I., II., III., etc. = passages.

TABLE I.

	A	*B*	*C*	*D*	*E*	*F*
I.	31.4	32.8	34.4		28.2	42.4
II.		36.2	35.4	36.8	29.4	44.8
III.	32.2		36.2	38.8	30.6	47.2
IV.	32.2	34.6		42.2		
V.	34	32.4	39	35.8	29	46
VI.	34.4	35.6	36.6	41.4	34.6	44
VII.	33.4	35.2	36	42	36.2	54.4
Av.	33.1	34.5	36.4	39.5	31.3	46.5
M. V.	.7	1.2	1.2	2.3	3.2	2.8

TABLE II.

I.	27.6	28.8	35.4	33.8	24.4	29.8
II.	27.8	29.4	37	33.6	24.6	31.2
III.	29.4	31.2	39	36.6	26.4	31.2
IV.	26.4	29.8	36.4	35.6	25.6	29.4
V.	25.4	28	35	33.6	25	41.8
VI.	29.6	21.6	36.6	35.4	28.4	38.4
VII.	27.8	30.8	37.6	37.6	31.2	45
VIII.	31.2	32.2	42.2	41	32.4	45
IX.	31.2	24.6	29.6	29.2	24.6	36.4
X.	29.4	28	33.8	34.6	24.4	48.8
Av.	27.3	29.4	36.3	35.1	27.1	37.7
M. V.	1.8	1.7	2.4	2.1	2.1	5.9

TABLE III.

	Av_1	Av_2	M. V.$_1$	M. V.$_2$
A	2	2	1	2
B	3	3	2.5	1
C	4	5	2.5	5
D	5	4	4	3.5
E	1	1	6	3.5
F	6	6	5	6

TABLE IV.

	A	*B*	*C*	*D*	*E*	*F*
I.	2	3	4	5	1	6
II.	2	4	3	5	1	6
III.	2	3	4	5	1	6
IV.	2	3	4	5	1	6
V.	3	2	4	5	1	6
VI.	2	3	4	5	1	6
VII.	1	2	4	5	3	6
VIII.	1	2	5	4	3	6
IX.	5	1.5	4	3	1.5	6
X.	3	2	4	5	1	6

TABLE V.

	Av.	M. V.		
1	19.6	.85	1.5	3
2	27	.3	3	2
3	22.2	.65	7	1
4	27.8	2.45	9	10
5	27.6	1.35	8	6
6	23.1	1.1	4	5
7	19.6	.95	1.5	4
8	26.8	1.85	6	8
9	28.3	1.95	10	9
10	23.5	1.7	5	7

TABLE VI.

	C	*N*	*R*
1	21.8	15	.69
2	19	14.4	.75
3	24	14	.58
4	17.2	16.2	.94
5	23.4	16.4	.71
6	27.4	15.6	.57
7	25	21.4	.86
8	24	17.2	.72
9	26.2	17.4	.66
10	24.2	13.4	.54
11	18.8	16.4	.87
12	23.6	18.4	.78
13	28	19	.67

thought; the remainder are from works of fiction. The passages are all one hundred words in length. Each subject first read aloud V., VI., VII. at subjectively normal rate, also three of I., II., III., IV. as indicated in the table. The fourth was read as rapidly as possible. Upon this occasion these passages were seen for the first time. On a subsequent date the subject read the passage previously read at rapid rate thirty-seven times in succession as rapidly as possible, and on subsequent dates each of the other first four passages was so read forty times, giving in all a practise curve of forty most rapid readings of each of passages I., II., III., IV. Later the subject again read at subjectively normal rate the whole ten passages, VIII., IX. and X. being now seen for the first time.

Table I. gives in seconds the time, average and M. V. of the six passages read at the first sitting, and Table II. gives the time, average and M. V. for the ten passages read at the second sitting, all at subjectively normal rate.

In both series there will be observed a general tendency for both average and M. V. to increase as we go down the scale. In Table I. the averages increase regularly save for the important exception of subject *E*. With this same exception, now in the other direction, the M. V.'s also increase regularly. The situation is reversed for *C* and *D* in Table II., but it has been noted that their positions are subject to a large probable error. However, if we compare *C*'s record in the two tables, the influence of diurnal variation would seem to be indicated. Subject *E* again stands higher than he should. His subjectively normal and most rapid rates of reading approximate one another more closely than is the case with the other subjects. The considerable practise with series I., II., III., IV. shortens all the repeated readings somewhat, whether among those practised or not. It will be noted that this practise has in subject *F* formed two species in the second readings, leading to an extra large M. V. The M. V. of I.–VI. is very small, but that of V.–X. is very large, larger than that of any of the other subjects, as is also the average of the two M. V.'s. Other results of the experiment, as the ratio of the normal and rapid reading, etc., do not appear to correspond so closely with the empirical series. Table III. gives the results in synoptic form by stating relative positions instead of the figures themselves. Thus *E*'s average was quickest for both series of readings, while his M. V. was greatest in the first and shared third and fourth places with *D* in the second. On the whole, the first readings are better than the second, and except for *E* the average is superior to the M. V., though both should be known. The character of the practise curves seemed to be related to other mental traits in the individuals, but had no traceable connection with the intellectual. Table IV.

gives the relative position of each subject in the rate of reading each passage. Thus I. was read by subject *E* fastest, *A* next and *F* slowest. Considering the small absolute differences, it seems worthy of note that the rates are so constant.

In reading aloud a wholly unfamiliar language, it would make no difference to our speed whether the words were in logical order or not. In so far as we did derive their sense should our speed with the logical order be increased over the illogical. Upon this theory a test was devised in which the subject read as rapidly as possible series I.–IV., and then read as rapidly as possible the words composing them in nonsense order. Nonsense syllables would have been preferable, as the writer now realizes. The subjects differed from those in the first test, only positions 1 and 2–3 being fairly certain. The differences are also very much smaller, and for the most part wholly subjective in estimate, as it did not appear worth while to obtain accurate judgments of relative position from a number of graders. The ratios of the two times, while showing much individual difference, appear so entirely unrelated to intellectual performance that it has been deemed hardly worth while to print them. They range from .47 to .97. Table V. shows in the first and second columns the results of this experiment for average and M. V., in the third and fourth the relative positions for average and M. V. The differences are not nearly so well marked as in the previous test, but the method should hardly be discarded on this account, for the number of observations and the differences in the subjects are very small, while correlation is by no means altogether absent. Striking exceptions to the rule, as *E* in Tables I. and II., are subjects *4* and *7*.

During the course of the *Psychometrische Untersuchungen* the observation was made by Cattell that a comparatively illiterate porter used as a subject required a longer time to read the name of a color than to name it, while the cultured subjects could read the name more quickly than they could name the color. The writer arranged a chain-reaction experiment with the four colors, red, green, blue, yellow, fixing twelve red and twelve green, thirteen blue and thirteen yellow slips upon a white background in a frame, while two similar frames contained the names of the colors in opposite orders. The color slips were so cut and arranged as to occupy the same average space as the names. The time was taken which the subject required to read the colors, and then their names in reverse order, and the ratio calculated. This experiment was performed upon thirteen subjects partly identical with those who had undergone the previous tests, and covering about the same range of intellectual ability as in the first test described. As shown in Table VI., all subjects name the words faster than the colors. If we disregard the ratio, and take

the time in either test singly, the experiment is brought into line with the speed of reading tests. For a series of single observations, the result is rather suggestive. Important isolated exceptions occur for the colors in subjects *6* and *11*, and in *10* for the names. The ratios, however, are simply chaos, just as were those in the previous experiment. The writer has examined the ratios with reference to other traits in certain of the subjects, but no correspondence with any factor of character is observable. The two perhaps most temperamentally similar subjects of the experiment—*3* and *4*—are nearly the most different in their ratios. There seems to be good theoretical ground to discredit the ratio as a desirable form in which to state results of this nature, for besides being highly subject to attenuation, it puts a premium on the subject's doing badly in one of the tests.

Most of the results are therefore negative. Two factors appear related to intellectuality: the speed of absolutely controlled association and its constancy relative to that of other individuals over a similar series. High speed and low variability go with intellectuality, low speed and high variability with the reverse; both high or both low are doubtful, but seem to be associated rather with the latter. To all these generalizations, however, one must be prepared for striking individual exceptions.

Frederic Lyman Wells.

Columbia University.

REVIEWS AND ABSTRACTS OF LITERATURE

Die Erkenntnistheorie der Naturforschung der Gegenwart: H. Kleinpeter, Ph.D. Leipzig: Johann Ambrosius Barth. 1905. Pp. xii + 156.

"The work before us agrees in general with the views as to the nature of knowledge expressed by Mach, Kirchhoff, Hertz, Stallo, Clifford, Pearson, Ostwald and Cornelius" (p. xi). It is dedicated to Ernst Mach. A theory of knowledge has developed in the second half of the last century on the basis of the exact sciences, and the author proposes to give it a much-needed systematic presentation. His method is neither historical nor critical, although he assumes a very critical attitude toward the science of metaphysics and the traditions of philosophy. Our only means of testing the correctness of views is through further experience, and this is a merely negative test, telling us only what is not true, but never what is. "Where this test does not apply, as in judging philosophical opinions, all genuine progress has been wanting for more than

two thousand years" (p. 2). Opinions which can not be tested by experience are only hypotheses; those which can be so tested are theories (p. 127). Philosophy in the sense of metaphysics is made up of hypotheses and lacks all scientific value.

The new theory of knowledge is characterized in three directions. First, in its insistence upon exactness, understood as a complete statement of the conditions under which its propositions are valid. "The form of its statements is therefore not the categorical, but the hypothetical" (pp. viii and 120 f.). Secondly, in opposition to the *Uebergriffe* of the mechanical view of the world, the newer view confines the investigator to the sphere of the experienceable (p. viii). It is the phenomenalistic point of view and follows as a necessary consequence from the principle of exactness. Lastly, the new theory of knowledge believes in 'a careful investigation of the meaning of our own words and constructions' (p. ix). This also follows from the principle of exactness, and this is the particular task of the author's book.

All knowledge is relative. There is no unconditioned absolute knowledge. Hence the possibility of the theory of knowledge as science. It is quite impossible to build a conception of 'thing-in-itself.' All fundamental knowledge bears in itself the form of a relation. A system of truths existing in and for itself and quite independent of the subject is impossible. Every truth or knowledge appears in the first place as the production of some individual. "Subjective conviction (*Ueberzeugung*), not objective certainty, is the only attainable goal of science" (p. 9). Moreover, the problem of science is no other than to be helpful in the acquisition of knowledge. A scientific work is not a magazine of finished and definitely formulated knowledge, but a guide by which to get knowledge—somewhat as a cook-book does not contain the eatables themselves, but guides us in the preparation of them (p. 13). Science mediates knowledge by showing that with the acceptation of some truths the acceptance of others is bound up, but science says nothing as to the necessity of accepting truth in general. We possess certainty in one case upon which such a compulsion rests, namely, immediate experience, the living through any content of consciousness. Science tells us that other certainties are bound up with this immediate certainty of experience. The author wishes to lay before the reader, first, the facts which underlie knowledge; then, a formal conception of knowledge based on these facts, and then its meaning or application. This is the method of physics, and, indeed, of all science properly so called.

"The first fact which meets us is that of the psychic nature of all facts" (p. 18). The physical is at bottom only a few sensations which we by association supplement and project into an 'external' world. "What I have seen is indeed only a few visual sensations" (not a person or a substance) (p. 21). "Thus we see that in our most ordinary expressions we are *arge Hypothesenschmiede.* On the ground of a single visual sensation we propose the hypothesis of having seen a man!" The senses never deceive us, but when we imagine that the senses give us a knowledge of objects error becomes comprehensible. Quoting Mach: "'Not

things (bodies), but colors, tones, pressures, spaces, times (what we usually call sensations) are the proper elements of the world'" (p. 22). Among the elements of consciousness we distinguish, first, the different types of sensation; secondly, the memory of these; third, complexes constructed of these two; and fourth, the feelings. The last give unity to consciousness and to the objects which make up its content. Feelings are nearer to us than the other elements, and this is the first important motive for constructing the conceptions of 'I' and the 'outer world.' The 'I' is, however, no mere sum of conscious contents, and is not itself an element of content. The author speaks of the self-activity of the ego (*des Ich*). Other names for it are 'spirit,' 'soul' and 'will,' and 'thought is a function of the will' (p. 32). The 'Ich' is always able to place itself over against its own conscious contents, to manipulate these, observing them more sharply or letting them pass into the background, to analyze them and to compare their parts with each other. This is the 'activity in consciousness' which the author regards as an indisputable fact of fundamental importance. Owing to the feeling of this activity which we possess, the activity itself introduces new elements into the content of consciousness and gives rise to the distinction between given facts and the pure constructions of my spirit (*Willenshandlungen*). The former appear in consciousness without the activity of the mind to assist them; indeed, they appear in spite of it. The latter, I myself bring forth.

'The justification of any concept, then, springs from its availability' or utility (*Benutzbarkeit*) (p. 38), and conception can only deal with actual and possible contents of consciousness. Protagoras was right in interpreting the expression, 'man is the measure of all things,' to mean that the activities and attributes of the human race are the measure of all things. Objective truth or knowledge is a secondary product. Each knowledge has significance primarily only for the individual who produced it. At bottom I know absolutely nothing as to whether outside of me there are other egos or not. My life confines itself to the circle of my consciousness (p. 43). Every act of knowing is a will-act, and thinking is dominated by considerations of economy and simplicity. The activities in which the thinking of the individual consists are the distinguishing of the different, the apprehension of relations of similarity between the different, the analysis of the discovered content to reduce similarity to equivalence, and the synthesis of previously separated elements.

Knowledges are classified first as formal and historical, the former dealing with properties of our memory pictures and imaginings and of all such contents of consciousness as we are able voluntarily to produce, the latter dealing with those contents which present themselves in entire independence of our wills. The formal sciences include logic, arithmetic, the science of combinations, geometry, kinematics, dynamics and and so forth; while the historical, which seek to establish facts or events independent of the will, include the history of humanity, the history of the earth, of the world, descriptions of single objects, geography and to some extent natural history. The real sciences constitute a third class

dealing not with facts for their own sake, but with regularities, uniformities and laws. They are physics, chemistry, biology, psychology. Certain typical sciences are discussed in order to get at the principles underlying them. The author treats logic, the theory of number, physics (its principles, presuppositions and goal), the value of hypotheses (as distinct from theories) in physics, and the natural view of the world. All knowledge, even that of logic and mathematics, is hypothetically, not categorically, certain. "It is possible to put forth many theories as to one and the same realm of facts, theories all of which satisfy the demands of admissibility and correctness and are nevertheless distinguishable from each other" (p. 112 f.).

Knowledge has a greater worth practically than theoretically. The discrepancy between the demands of practical living and the limits of strictly scientific investigation gives rise to philosophy, a system of hypotheses which can not be experienced or tested by experience. Hungry men do not wait for science to demonstrate the nutritive value of bread. It is necessary, however, to keep distinct the line of demarcation between science or theory and hypothesis. We owe it to a lucky accident merely that many of our hypotheses define conditions which practical life fulfills. Their fulfillment is something we have not the slightest theoretic reason to expect.

This book is typical of the movement which it represents, in that it voices a demand for the unification of knowledge in a critical theory of the presuppositions and methods of science. One of the most striking characteristics of the development in exact science in the latter half of the nineteenth century is the consciousness of this demand. The author is correct in saying that one of the distinguishing peculiarities of recent science is the attempt to criticize its own presuppositions and constructions. It is characteristic of the movement, too, that it seeks unification and the removal of discrepancies, not in a hypothetical metaphysic, but in a theory of scientific method. Understanding the term to include the theory of all method, we should say there is no other genuine unification of knowledge than this; and if we ask Kant's question, what kind of a world must ours be, seeing that it is knowable, we can only answer by pointing to the contents of the various sciences. For the mind to set about discovering the limits of knowledge by defining a reality which can not be known, as Locke did and as Kant resulted in attempting, is to be misguided by a contradiction. We are as likely to succeed in defining the space which lies beyond all space as in defining the 'being' of substance, *das Ding an sich* or a transcendent absolute.

But the theory of knowledge of this book is founded on what might be called the epistemologist's fallacy, the doctrine that 'all facts are psychic facts,' or, as Mach puts it, that 'not things (bodies), but colors, tones, pressures, spaces, times (what we usually call sensations), are the proper elements of the world.' This is reading into immediate experience (which the author regards as the basis and source of all certainty, the ultimate premise of reflection) certain distinctions which are the results of reflection. To say that all facts are psychic implies

that there is something else (*e. g.*, physical) which they might be. Sensations are scientific abstractions; they are not to be confused with the original datum with which reflection begins her task of interpreting immediate experience. It is because of this fallacy that the author is compelled to ascribe to science at last a much lower theoretic than practical value. If the only data of science were the sensational and memory elements entering into the content of consciousness, as Kleinpeter holds, solipsism would necessarily be the final word in our theory of knowledge, and the theoretic value of science would be even lower than our author supposes. For some of the theoretic values which he accepts presuppose the existence of other consciousnesses and other Leibnitzian worlds, a presupposition which, from the author's standpoint, admits of no sort of reflective demonstration.

Moreover, knowledge can possess validity, from this point of view, only for the moment when it arises. The author holds that by giving the conditions under which knowledge arises this difficulty is overcome. "The knowledge which in the first place possesses value only for the moment can acquire a lasting value through the citation of the conditions under which it retains validity (*durch die Angabe der Bedingungen unter denen sie Gültigkeit behält*)." But the author does not maintain that the elements of consciousness (sensations, memories of sensations, complexes of these, and feelings) are things which perdure. He holds that things are reducible to sensations, etc., but the reverse proposition that sensations are things is another matter. He recognizes the intermittent and transient character of the elements of consciousness. What, then, from this point of view, can the conditions under which knowledge retains its validity be? Practically knowledge has value, but a value which we have, on this basis, absolutely no reason to expect and no means of justifying.

Some other points may be barely mentioned at which the author's treatment seems to the reviewer inadequate. His doctrine as to the function of science, that science, like a cook-book, guides us in the attainment of knowledge but does not itself contain or give us the good things on which the intellect subsists, does not convey a definite meaning. Science is a guide in the attainment of further experience, but this is the function of all knowledge, and experience is richer than knowledge. His treatment of objectivity, space and perception seems to the present writer very incomplete—so much so that we get no definite idea from it. His conceptions of immediate experience, and of the will-acts through which knowledge arises, are extremely vague. In the sense in which he uses the term, immediate experience is only a name for the abstract quality of immediacy, and the experiences he seems to regard as immediate (sensations, etc.) are themselves abstractions. The distinction between the physical and the psychical is not clear. The work, in short, is too brief, and although well-conceived, too schematic to be persuasive.

G. A. Tawney.

Beloit College, Wisconsin.

Lectures on the Method of Science. Edited by T. B. STRONG. Oxford: The Clarendon Press. 1906. Pp. viii + 249.

A course of lectures on scientific method was delivered at Oxford during the Summer Meeting of 1905 at the request of the Delegates for the Extension of University Teaching. Nine of the lectures have been brought together in the present volume under the editorship of Dr. Strong, Dean of Christ Church, who contributes himself the closing lecture on 'Scientific Method as Applied to History' and also a preface explaining the general aim of the course—which was to illustrate the 'forms taken by scientific method in various departments of research.' The remaining lectures are as follows: 'Scientific Method as a Mental Operation,' Thomas Case, M.A., President of Corpus Christi College and Waynflete Professor of Moral and Metaphysical Philosophy at Oxford; 'On some Aspects of the Scientific Method,' Francis Gotch, D.Sc., F.R.S., Waynflete Professor of Physiology at Oxford; 'Physiology; its Scope and Method,' C. S. Sherrington, D.Sc., LL.D., F.R.S., etc., Professor of Physiology, University of Liverpool; 'Inheritance in Animals and Plants,' W. F. R. Weldon, D.Sc., F.R.S., Linacre Professor of Comparative Anatomy at Oxford; 'Psychophysical Method,' W. McDougall, M.A., Wilde Reader in Mental philosophy at Oxford; 'The Evolution of Double Stars,' A. H. Fison, D.Sc.; 'Anthropology: The Evolution of Currency and Coinage,' Sir Richard C. Temple, Bart., G.C.S.I., D.C.L.; 'Archeological Evidence,' W. M. Flinders Petrie, D.C.L., F.R.S., Professor of Egyptology, University College, London.

This array of titles and lectures is sufficient to provoke admiration and interest at the outset and to pitch the reader's expectations rather high. Dr. Strong's preface, although written with noticeable caution, adds to one's anticipations. For we have in this Oxford course an attempt to meet a demand felt by educators and investigators alike to bring a little more unity into the too detached departments of human knowledge. It is socially and educationally significant that this attempt should have been made at an Oxford Summer Meeting under the auspices of the university extension movement, because the circumstances indicate—at least in the minds of those who planned the course—that interest in the unification of knowledge is quite general, and not the exclusive possession of students of logic. It is philosophically significant that the unification should have been attempted through a discussion of scientific method concretely illustrated from various departments of research because it suggests that science is essentially not a body of knowledge, but a method of investigation and an instrument by which nature may be measurably controlled. Skilled in the use of this method and instrument, one might face the varieties of knowledge with composure and, possibly, with confidence. One's conquests would tend to promote at least an administrative unity.

The present volume, however, depends too much upon its title and its preface. Uninspired by their suggestions, the reader would not suspect that he was following a course on scientific method. He would rather suppose that he was receiving an amount of very interesting and very

miscellaneous information. Professor Case's lecture is a brief treatise on inference by analogy, by induction and by deduction, and departs very little from traditional logic. Such a treatment has become too obviously formal to throw much light on scientific method as a mental operation. Perhaps little light can be thrown upon it as such in any event, for scientific method appears to be not so much the mental operations involved in the making of inferences as a handling of material in a way to make inferences easy and obvious. If the title of Professor Gotch's lecture had been 'On some Aspects of the Scientific Temper,' it would have more accurately described the contents. They deal little with method, but much and interestingly with the temperamental characteristics of scientific men as these are revealed by the opposition science has encountered in its progress. The reader may be led to appreciate that for which science stands, but he will not be much wiser in its methods. Under the caption 'Physiology; its Scope and Method,' Professor Sherrington writes most entertainingly on the heat of the body, but in spite of ingenious suggestions and occasional reminders he can hardly expect the reader to believe that he is thereby illustrating the method and scope of physiology. One attending the Summer Meeting would have been unwise, however, if he had avoided the lecture on that account.

Professor Weldon's contribution invites the same criticism of sailing under false colors, but with a kind of perversity illustrates scientific method better perhaps than any other lecture in the volume. Inheritance in plants and animals is his announced theme. He says, however, practically nothing about it. Instead he expounds the method of statistics as illustrated in measuring the latitude of the Radcliffe Observatory, in determining the weight of oxygen and nitrogen, and in the throwing of dice. A single paragraph toward the close of the lecture states Pearson's result from applying the same method to determine the regression between parent and child as illustrated by the relation between breadth of span in mothers and in daughters. But we have a genuine discussion of scientific method concretely illustrated. It is doubtful whether the discussion does much to clarify the method, but it produces the conviction that the scientist has at his command the means of bringing extremely variable phenomena within the domain of measurement.

Perhaps it was the feeling that the term 'psychophysical method' is a misnomer that led Professor McDougall to devote his lecture to a consideration of psychophysical science in general, its relation to its allied sciences and to an illustration of the way the psychologist has met the difficulties encountered in determining the action-time of light. This illustration, which is worked out with admirable brevity and clearness, together with an equally excellent exposition of Fechner's conception of the requirements of psychophysical science, can not fail to give the reader a lively appreciation of the complexity of the problems with which the experimental psychologist has to deal and of the means he has employed. A region of investigation is thereby determined, but not a peculiar method which should bear the name 'psychophysical.' The methods employed are common to all workers in science. The problem only is distinctive.

Yet the lecture admirably illustrates the way the scientist by the use of experiments can disentangle factors which appear to be inextricably interwoven. It exhibits the method of experimental analysis in operation. Although the method receives no general formulation, it was doubtless worth while to behold it concretely at work in one of the more recent departments of research.

A result similar to Professor McDougall's has been achieved by Dr. Fison in his lecture on the evolution of double stars. It is a highly interesting and instructive lecture. Indeed the part played by the investigation of double stars in forming our general conception of stellar evolution is so admirably told that the problem of method is obscured by the beauty of the result. Yet the lecture merits study because of the richness with which it illustrates the method of employing the mechanics of fluids in the interpretation of celestial phenomena. Perhaps no department of science can offer more striking instances than that from which Dr. Fison has drawn of the transition from experiment to generalization in a new region and back again to experiment. Here we find at once the familiar and the powerful procedure of science. The familiar operations of ordinary things once formulated in mechanical terms become themselves instruments for analysis as powerful as the more material tools and devices to be found in any laboratory.

In Sir Richard Temple's lecture on the evolution of currency and coinage as an illustration from the science of anthropology, the question of method is once more lost—and this time entirely—in subject-matter. To be sure, we are told that the evolution exhibits the 'constant nature of human reasoning' and the 'law of contact' which is 'the fundamental law to be observed in all anthropological research.' The bearing of these considerations on scientific method is not obvious.

Professor Petrie in discussing the types of evidence with which the archeologist deals approaches once more the general theme of the course. For evidence is a device to make inferences easy and obvious. The four types of evidence noted—witnesses, material facts, exhaustion, probability—are illustrated in the realm of law and parallels drawn in the realm of archeology. Certain factors which tend to corrupt evidence are noted, but there is little suggestion of a genuine methodology.

Dr. Strong in the closing lecture considers, first, the 'form of the historical statement and the historical question,' and, secondly, 'our mental attitudes towards historical statements.' The reader has once more his general question put to this particular case: How do such considerations illustrate the scientific method as applied to history? There is some examination of the general problem of evidence, and Dr. Strong distinctly opened the way for a discussion of general methods in specific application by distinguishing historical facts from those of physical science. The former belong to the class of facts which are unique and occur once, the latter to the class that are general and can be repeated. The historian's problem, according to Dr. Strong, is thus so to reconstruct the setting of the unique fact he is studying that this fact will appear rigidly placed. How this can best be done was worth more de-

tailed consideration than is here given to it, or might have been illustrated by concrete examples. Criticism of Dr. Strong should, however, accept his own view of the peculiar difficulties of his task: "This task appears to me difficult because, while much is said nowadays of the importance of a scientific conception of history, I do not think there is anything like the same agreement about the character of scientific history as exists in the case of most other sciences."

"The course as delivered in Oxford was received with great satisfaction." So we are told. There can be little doubt of it. One can readily believe that the audience remained in generous attendance to the end. But it is hard to believe that they were much enlightened on the method of science.

FREDERICK J. E. WOODBRIDGE.

COLUMBIA UNIVERSITY.

Essay on the Creative Imagination. TH. RIBOT. Translated from the French by ALBERT H. N. BARON, Fellow in Clark University. Chicago: The Open Court Publishing Co.; London: Kegan Paul, Trench, Trübner & Co. 1906. Pp. xix + 370.

Mr. Baron has done us a service of some value in rendering into English M. Ribot's monograph on the creative imagination. The translation sticks somewhat closely to the original idiom, but this is a virtue rather than a fault. The book is neatly gotten up, well printed, with a good index. It forms a valuable addition to the psychological literature on imagination, and it is to be hoped that some of the other French monographs on kindred topics will receive a similarly respectable English dress. It is strange that some of the recent French monographs on attention, will, etc., should have been prepared in English, and then been done into French. This, however, is by the way.

I shall by no means attempt to give an adequate account of the contents of the book. As it has been translated into English, it is accessible to any one choosing to look up the subject further. In the following remarks I shall try rather to call attention to the striking features which are in evidence.

As the title indicates, the subject is restricted to 'Creative Imagination.' No discussion in full of memory or association is to be expected. But imagination as it is usually understood is treated in full. As M. Ribot insists, creative imagination is something more than reproductive imagination, or memory. "In imaginative creation we have several cooperating images, with combinations, coordination, arrangement, grouping" (p. 8). After having defined the subject, M. Ribot proceeds to discuss imagination under the following heads: Analysis of the Imagination, Development of the Imagination, Types of Imagination, Conclusion and Appendices.

Analysis.—As regards its intellectual aspects, imagination presupposes a negative operation, dissociation, and a positive operation, association. In the process of dissolution images may become incomplete, schematic, or they may remain more or less complete. In the association of such

images, the laws of contiguity (or continuity) and similarity operate, each having a specific method of its own. As a special form of resemblance, analogy is most in evidence in the processes of creative imagination. As regards the emotional factor, "all forms of creative imagination imply elements of feeling. . . . All invention presupposes a want, a craving, a tendency, an unsatisfied impulse, often a state of gestation full of discomfort" (p. 32). This impulse then takes a more or less definite form under guidance of a series of images. An unconscious factor exists in what is usually termed 'inspiration.' By association, mediate or otherwise, some form, complex or series of images is evolved, and flashes upon consciousness. But M. Ribot insists that such a state can not follow from any mental vacuum, but is rather the result of long and profound mental activity.

Development.—In the second section of the book development of creative imagination is taken up in a treatment of imagination (1) in animals, (2) in the child, (3) in primitive man and myth creation, and (4) in the higher forms of invention. A good idea of M. Ribot's full discussion of these topics may be had by reading carefully Chapters I. to IV. of the second part of the book.

Types.—The chief types of creative imagination M. Ribot finds in the plastic imagination, the diffluent imagination, the mystic imagination, the scientific imagination, the practical-mechanical imagination, the commercial imagination, and the utopian imagination. M. Ribot uses the term 'plastic' in a manner somewhat different from that of Professor Baldwin. M. Ribot considers plastic imagination that which makes use of clear images well defined in space, and guided by objective associations. It is used chiefly in arts dealing with form, as in poetry, myths and mechanical inventions. The diffluent imagination makes use of vague images loosely connected by association. It is manifested in revery, romantic dreaming, religious conceptions, literature and the fine arts. The mystic imagination is concerned chiefly with symbols, and exists in religion and metaphysics. The scientific imagination is most exacting since it must represent 'not only the elements of the past and the present, but in addition construct a picture of the future according to probable inductions and deductions' (p. 238). In addition a rigorous use of reason is necessary to give method to chains of images. "It is the imagination that invents, that provides the rational faculties with their materials, with the position and even the solution of their problems. Reason is only a means for control and proof; it transforms the work of imagination into acceptable, logical results" (p. 243). The practical and mechanical imagination gives rise to invention, the commercial imagination deals with schematic images, while the utopian is concerned with social and ethical problems.

Almost too sketchily have I given some of the features of the book. I have omitted almost entirely any mention of M. Ribot's excellent treatment of invention, of the organic conditions of imagination, of the principle of unity, and of the appendices and conclusion.

Underlying the entire discussion of imagination are the following

sound principles, as emphasized by the author: (1) In invention and creative imagination there is no general faculty. Only specific genius is possible as shown in stated instances. (2) Special conditions determine to a great extent the progress possible, and the validity of imaginative creations. (3) Imagination is not a power *in abstracto,* but is simply the interplay of material peripherally acquired, because of emotional impulsion.

As I have said above, it is impossible in this review to do more than suggest the method of treatment pursued by the author. The full explication is to be found in the book itself, which is now accessible to all through the translation of Mr. Baron.

FELIX ARNOLD.

NEW YORK CITY.

JOURNALS AND NEW BOOKS

THE AMERICAN JOURNAL OF PSYCHOLOGY. July, 1906, Vol. XVII., No. 3. *The Psychology of Organic Movements* (pp. 293–305): I. MADISON BENTLEY. – A critical survey of the recent tendencies in psychology to emphasize the motor side of consciousness, and a warning against the danger of making some aspect of it, *e. g.,* attention, the vague all-explaining entity that the conception of soul formerly was. *The Habits, Instincts and Mental Power of Spiders, Genera Argiope and Epeira* (pp. 306–357): JAMES P. PORTER. – The main point brought out by this study is the variability of the instincts of spiders. This is probably the basis for the development of new species, and possibly the starting-point for the development of intelligent action. *A Study of the Affective Qualities. I. The Tridimensional Theory of Feeling* (pp. 358–393): SAMUEL PERKINS HAYES. – The experiments made give no evidence for the tridimensional theory of feeling advanced by Wundt, but support the dual theory in its traditional form. *Accuracy in Handwriting as Related to School Intelligence and Sex* (pp. 394–405): ARNOLD L. GESELL. – Accuracy in handwriting is found to vary directly as school intelligence, and hence forms a very convenient test in the elementary schools. *The Effect of Music on Thoracic Breathing* (pp. 406–414): EUGENIA FOSTER and E. A. McC. GAMBLE. – Music (1) tends to make breathing faster and shallower; (2) has no effect on the regularity of breathing; (3) no pronounced differences shown between the effects of loud and soft and major and minor music. *Psychological Literature. Book Notes.*

Davidson, Thomas. *The Philosophy of Goethe's Faust.* Six lectures delivered in the winter of 1896 at Cambridge, Mass. Edited by Charles M. Bakewell. Boston: Ginn & Co. 1906. Pp. iv + 158. $0.60 *net.*

Fournier, E. E. *The Electron Theory.* London: Longmans, Green & Co. 1906. 5s.

Hobhouse, L. T. *Morals in Evolution.* Two volumes. London: Chapman & Hall. 1906. 21s.

MacColl, Hugh. *Symbolic Logic.* London: Longmans, Green & Co. 1906. Pp. xi + 141. 4s. 6d.

Macleane, Douglas. *Reason, Thought and Language; or the Many and the One.* London: Oxford University Press. 1906. 15s.

Morgan, C. Lloyd. *The Interpretation of Nature.* New York: G. P. Putnam's Sons. 1906. $1.25.

NOTES AND NEWS

At the meeting of the Aristotelian Society on November 8, the president, Dr. Hastings Rashdall, delivered the inaugural address, on 'Nicholas de Ultricuria, a Medieval Hume,' of which the following summary is taken from the *Athenæum*: "Dr. Rashdall began by suggesting that current impressions of medieval philosophy did scant justice to the originality and independence of the speculation which prevailed in the medieval schools, partly because the most famous doctors were the accepted theologians of the regular orders. These had exceptional facilities for getting their works diffused, read and taught throughout Europe, and eventually printed in massive folios, while the secular teachers were forgotten. In the case of the more unorthodox, successful persecution had so completely doomed their ideas to oblivion that their very names are hardly mentioned by historians of philosophy. A remarkable instance of this process is supplied by the fate of Nicholas de Ultricuria (of Autricourt, now Avricourt), of whose works nothing remains but two letters and the propositions which in 1346 he was compelled to retract. Yet the leading opinions of Berkeley and Hume were all anticipated by this fourteenth-century schoolman. Among the condemned theses (now published in Denifle and Chatelain's magnificent 'Chartularium Universitatis Parisiensis') the following were some of the most notable: 'Of the existence of material substance other than our own soul we have no evident certainty'; 'we do not know for certain that things other than God can be the cause of any effect'; 'we do not know evidently that any cause but God can exercise efficient causality.' He doubted, in short, the existence of matter, the existence of the self except as an effect of divine causality, the existence of any self-evident or *a priori* truth, the necessity of the causal nexus and the validity of any inferences based thereupon. In some ways his scepticism went beyond that of Hume himself: it reached its climax in the assertion that the only thing we can be certain of is, 'If something is, something is.' Nicholas represented, Dr. Rashdall thought, an extreme development of the empiricism of Occam, though his determinism was no doubt due to the influence of Bradwardine. In spite of all his scepticism, there was no reason to doubt that he was quite sincere in his Theism and his Christianity. What his speculation probably meant was that faith must be substituted for knowl-

edge as the basis of religious belief; yet he was not a mere spinner of ingenious metaphysical cobwebs, but a real thinker who had fairly entered upon the line of speculation ending in the doubts which, in the form given to them by Berkeley and Hume, all modern philosophy has been engaged either in meeting or confirming."

Henry C. Brockmeyer, who died in St. Louis on July 26 last, at the age of nearly eighty, was a noteworthy figure in the intellectual history of America as the first of the American Hegelians and the founder, in this country, of the systematic study of German philosophy. A native of Prussia, he came to America at sixteen, studied for a time in Georgetown College and in Brown University, settled in St. Louis, and, while employed as a moulder in an iron foundry there, in 1858, gathered about him a group of young men who began with him a careful study of the systems of Kant and Hegel. From this group, of whom William T. Harris was one, sprang the whole Hegelian movement of which St. Louis was the center. Mr. Brockmeyer was the first president of the St. Louis Philosophical Society (1866), which in 1867 began the publication, under Dr. Harris's editorship, of the first philosophical periodical on this side of the Atlantic—the *Journal of Speculative Philosophy.* To this Mr. Brockmeyer contributed two series of 'Letters on Faust' and (with Dr. Harris) a translation of Hegel's 'Phenomenology.' Combining in an exceptional manner practical force with speculative interests, Mr. Brockmeyer played an historic part in maintaining public order in an out-of-the-way section of Missouri during the Civil War, and after 1870 was active in political affairs. He had the principal part in the framing of the Missouri constitution of 1875; and in 1876 was elected lieutenant governor of the state. Dr. Harris once wrote of him: "Mr. Brockmeyer was a thinker of the same order of mind as Hegel, and even before reading Hegel, except a few pages in Hedges's 'German Prose Writers,' had divined Hegel's chief ideas and the position of his system." Mr. Brockmeyer left in manuscript at his death a complete English translation of Hegel's 'Greater Logic'; it is hoped that means may be found for its publication.

The Philosophical Union of the University of California has been carrying on for the past year a series of studies introductory to the philosophy of religion, the success of which in awakening interest has been such as to determine the union to continue its work in this field during the present year. Professor McTaggart's 'Some Dogmas of Religion' has been chosen as the basis of discussion. At each meeting a paper will be presented, to be followed immediately by discussion which shall be opened by an appointed leader. Attendance at these meetings is not limited to members. The following program is announced: November 23, 'The Necessity and Ground of Dogma,' Professor C. H. Rieber; December 14, 'Free Will,' Rev. R. P. Shepard; January 27, 'Human Immortality,' Dr. F. L. Wrinch; February 15, 'Human Preexistence,' Professor G. H. Howison; March 29, 'God as Omnipotent,' Dr. M. E. Blanchard; April 26, 'God as Non-Omnipotent,' Professor J. W. Buck-

ham; May 10, 'Theism and Happiness,' Dr. W. E. Hocking. Professor McTaggart will make the annual public address on August 23.

THE anthropological and psychological sections of the New York Academy of Sciences met on November 26. The afternoon session was in the psychological laboratory of Columbia University, and the program was as follows: 'Linguistic Ability and Intellectual Efficiency,' Dr. F. Lyman Wells; 'Esthetics of Simple Color-arrangements,' Dr. Kate Gordon; 'Gustatory Audition,' Professor A. H. Pierce; 'The Pendular Whiplash Illusion of Motion,' Dr. Harvey Carr. At the evening session, held at the American Museum of Natural History, the following papers were read: 'Imaginative Thought as Adaptive Response,' Professor Robert MacDougall; 'Psychology and Spelling,' Brother Chrysostom; 'Knowledge and Judgment,' John Dewey.

Cœnobium, Revue internationale de libres études is the name of a new journal for the promotion of liberal and speculative interests, to be published at Lugano, under the direction of Signore Giuseppe Rensi. The managers find their opportunity in the present shifting of metaphysical attitudes, and hope to contribute to the coming readjustments in philosophy. It is not intended that *Cœnobium* shall favor one philosophical position more than another.

ANOTHER new periodical in the field of philosophy is the *Rivista Rosminiana,* of which the first number appeared in July. Unlike *Cœnobium,* the *Rivista Rosminiana* will champion a particular line of philosophy, namely 'spiritualismo cristiano,' of which Rosmini was one of the most distinguished representatives. The first article, 'La Filosofia dell' azione e l'apologetica moderna,' is a study of pragmatism, with special reference to the theories of James. The *Rivista* is published at Lodi, and edited by Professor Giuseppe Morando.

THE American Philosophical Association, Professor William James, president, Professor John Grier Hibben, secretary, and the American Psychological Association, Professor James R. Angell, president, Professor William Harper Davis, secretary, will meet December 27–28, in connection with the meeting of the American Association for the Advancement of Science, in New York City.

THE second meeting of the Southern Society for Philosophy and Psychology will be held in Montgomery, Alabama, in connection with the Southern Educational Association, December 27–29.